

This question of the reason of symmetrical organs having different functions might perhaps be elucidated by a study of the conditions under which deviation from bilateral symmetry occurs in the structures of animal life, even among the highest arthropods and vertebrates.

P. S.

May 23

The "Chromatic Octave"

RENEWED attention to the "chromatic octave" tempts me to suggest an experiment. There used to be a gentleman, Smith, I believe, by name, who refuted the undulatory theory by means of a disc, divided into black and white sections, which he whirled with very high velocities, producing colours (so the *Times* positively stated) varying according to the velocities. It is plain that such a result might on the contrary confirm the theory, if, for instance, the disc were divided into 400 black and 400 white sectors, and whirled at the rate of one or two million million times a second. It is also plain that Mr. Smith, in the words of an authority who has been quoted in your columns for weightier judgments, would have blown his disc into smoke first. But *once* a second is only 40 octaves below a million million times; and it is just possible that something practicable between the two might throw light on the "chromatic octave," among other things. There are some obvious objections: the question is, whether they make it worth while to repeat Mr. Smith's experiments.

I have just received the number of *Poggendorff* (Oct. 1867, CXXXI.) containing Professor Listing's paper (p. 564), referred to by Mr. Barrett in *NATURE* for Jan. 13 and March 31. I did not expect it would support Mr. Barrett, but I was surprised to find how directly it contradicts him. "Listing," he says, "concludes that, although physiologically and psychologically there may be differences, yet there is an indisputable physical basis for the analogy between tones and colours." I had myself, at the end of my remarks in *NATURE* for Feb. 3, admitted the "physical basis" in that sense "of the word *physical* which excludes biological relations," and the remark is too trivial to have formed Listing's conclusion. It forms part of a sentence in the first page of the paper. "The analogy between tones and colours, which has often been pursued with excessive predilection, and which certainly has an indisputable physical basis, has against it numerous points of disagreement (*Discongruenzen*), even now not in general sufficiently attended to, which depend rather on the physiological and psychological aspect of the phenomena." In the same page of the number it clearly appears how much is meant by the *physical basis*. "Physically," he says, "it is the period of vibration that determines both tone and colour; but the physiological effects stand in very different relations to the common element in the two cases." He proceeds to show, as correctly explained by Mr. Barrett, that the several colours divide the spectrum in an arithmetical progression of their rapidities of vibration; and at the end of the paper, contrasting this phenomenon with the geometrical progression of a uniform series of tones, he says: "This point of disagreement, a very vital one in my opinion, between the scales of tone and colour, may be briefly stated thus: *In the musical scale* (chromatic and with equal temperament) *the logarithms of the tones are in arithmetical progression, in the scale of colour the colours themselves.*" That this should mean what Mr. Barrett understands it to mean, you must read *agreement* for *disagreement*.

Of the reality of Listing's result, I suppose there can hardly be any doubt; and I am glad that Mr. Barrett has corrected my suggestion that it probably represented a conventional demarcation. There does seem something arbitrary in the number of divisions made, but their positions represent a mean among the impressions of different observers as to the boundaries between colours answering to the names assigned; and the accuracy of these determinations may be fairly estimated by likening them to the case of a person who, having to divide a space of nine inches into nine equal parts, should be correct *as often as not* within one 24th of an inch. But the most important point is this, that the observers would not be aided, but must rather have been distracted, by the spaces actually occupied by the colours in the spectrum. For the observations were made on two different spectra, the irregular one obtained from the prism, and the diffraction spectrum in which the colours proceed uniformly by wave-lengths; and the result was a division into equal spaces, not on either of these visible spectra, but on the ideal spectrum, which should proceed uniformly by rapidities of vibration. It

* If this is what is meant by "chromatischen gleichschwebenden."

would have been in the spirit of good German precedents if we had been given some measure of the variation between different observers.

It must be confessed that all this is damaging to the theory of a "chromatic octave," essentially a theory of geometrical progression. Still more obviously damaging is the fact that "lavender" would be the octave above something so unlike it as "brown," or "brown" and "red."

Mr. Murphy's argument (*NATURE*, April 28) seems to assume that complementary wave-lengths must be in *some* constant ratio. His theory is, at any rate, inconsistent with his author's; for primary red and blue would be nearly complementary, so that "true white" could not be produced by any mere preponderance of blue, and would be white only to the green-blind.

May 9.

C. J. MONRO

In Mr. Murphy's interesting letter in No. 26 of *NATURE*, April 28, 1870, he assumes that the number expressing the frequency of vibration producing a colour complementary to another, is the geometrical mean between the frequency of vibrations corresponding to that other, and its double. By this means he does not get colours complementary from sunlight. Thus red and bluish green (whose numbers are respectively 36.4, 48.3) are not complementary on his hypothesis; which would require the number for bluish green to be 51.47. So for yellow and indigo, the numbers are 41.4, 54.7, but should be 41.4, 58.4. This he attributes to the impurity of the solar spectrum. There seems as much reason, however, for taking the *harmonic mean* instead of the geometric; and, on this supposition (the harmonic mean between two quantities being twice their product divided by their sum), the numbers would be red, 36.4; bluish-green, 48.5; yellow, 41.4; indigo, 55.2. The second and fourth, 48.5, 55.2, are not very different from 48.3, 54.7. Taking then a colour twice over in the spectrum and its intermediate complementary, the relation between the three would be that of a musical note, its fifth and its octave.

Little Wratting, Suffolk, May 16

M.A.

The Colour of the Moon by Day and by Night

CAN any of your readers give me a full explanation of the reason why the moon looks white by day and yellow by night? The light that proceeds from it is of course the same at both periods; whence does the change in appearance arise? Two reasons occur at first thought, but they do not completely satisfy the many requirements of the problem. The one is, that the light, being really somewhat yellow, though less so than it often appears to be, passes in daytime through an atmosphere made blue by the solar rays, and the blue and yellow neutralising each other, the moon looks white. The other reason is, that as the evening closes in, the twilight becomes purple, and the moon being but moderately yellow in itself, looks more intensely yellow by contrast. All this is correct so far as it goes; but I do not see why the moon should often look extremely yellow in the middle of the night after twilight has quite disappeared. Does it show that the light, one knows not exactly whence it comes, which is found even on clouded and moonless nights, is purple? There are some grounds for this hypothesis, because the moon almost always, as I have been assured by a practical astronomer, looks comparatively white through a telescope, which of course isolates the field of vision. Also, it seems to me that the street gaslights are just as yellow at midnight as in twilight; the stars, also, commonly look yellow all the night through. It is strange that the very frequent and beautiful phenomenon of the white moon of the day suddenly turning yellow as the evening closes in, should not have long since attracted scientific comment.

F. G.

What is a Boulder?

A CORRESPONDENT in your journal of the 26th of May inquires about the size of boulders, and states that he cannot find any definition of the word which gives a notion of its size accurate enough for scientific purposes.

There are several definitions of boulder-stones given by geologists and others, which determine their size within tolerably narrow limits.

Dr. Page defines boulders as being "any rounded or water-worn blocks of stone, which would not, from their size, be regarded

as pebbles or gravel;" or generally, "any *large* water-worn and smoothed blocks, found embedded in the clays and gravels of the drift formation." The maximum size he assigns to gravel is the size of a hen's egg, and pebbles may be ten times as large as this, so that the smallest boulder may be regarded as being of more than a foot diameter. Again, the same author describes boulders as being "often of great size, and weighing many tons."

Dr. Nuttall, in his Pronouncing Dictionary, defines boulders as being "large round pebbles;" and it would not often be possible to put even a small pebble in one's waistcoat pocket, and it must be a high wind that could blow a large pebble for any distance.

Chambers gives, as the etymology of boulder, or bowlder, as it is sometimes spelt, the verb "to bowl," and so states that it means a rolling stone, and, afterwards, one rounded by water. Now boulders are very often found separate, and at some distance from each other, and how could they have been rounded separately, without friction with each other, if they had been otherwise than large?

These arguments may be enough to show that boulders cannot be very small, and, in fact, may vary from stones 18 inches diameter, or thereabouts, to immense rocks of 10 feet diameter, "weighing many tons."

Christ's Hospital, May 27

J. W. CRAWLEY

Scandinavian Skulls

MR. G. STRACHEY, commenting in your columns on a recent lecture of Professor Huxley's, makes the following statements:—

"According to the highest Copenhagen authorities, there is no ground whatever for the assertion that modern Scandinavian skulls are of the long type. It is equally incorrect to say that Scandinavians are fair-haired and blue-eyed."

Without at all wishing to endorse Prof. Huxley's views on British ethnology, which I regard as rather bold than sound, I may be allowed to express my astonishment at the statements just quoted.

The skull measurements of Retzius, Van der Hoeven, and Barnard Davis all exhibit the Swedes as a more or less dolichocephalous people. My own measurements of the living head (Anthrop. Memoirs, ii. 351, and iii. 378) tend to controvert both Mr. Strachey's facts and Prof. Huxley's theory. Lastly, I should be glad to know where in the world fair hair and blue (or light) eyes are to be found, if not in Scandinavia. They are not universal there, nor any where else, but I do not think they are anywhere more common, except possibly in Lithuania or Esthonia.

Clifton, June 7

JOHN BEDDOE

Formation of Caverns

IN NATURE of 21st April, Mr. W. Boyd Dawkins, writing of caves in Yorkshire, tells us:—"All have been at one time or other subterranean water-courses." In the *Popular Science Review* for October 1869, the same gentleman writes:—"The ceiling, at the time of its deposition, must have been supported by a layer of cave earth." With your permission I will explain the character of a phenomenon, which I have published in my "New Pages of Natural History" (Newby, 1868), which may suggest to Mr. Dawkins the manner in which the caves at Streddle and Kent's Hole were formed. In the province of Poona, Bombay, the Ghar Nuddee (white river) has, in the mountains near its source, several lime formations spanning its level. In the dry season the river runs below as a subterranean stream; in the Monsoon it runs over this sheet of lime, which varies in thickness from a few inches to two feet along the centre or crown of the cavity, increasing in thickness towards the sides; there are several fissures on the surface; the hole at the upper end is smaller than that at the lower; the whole formation is in layers, and is due to water containing vast quantities of lime in solution. Originally there was a dip or hollow place on the spot, which gradually filled with all sorts of materials, till they grew nearly to the level of the dry season stream. In this condition the first thin sheet of lime was deposited on them, till by successive seasons the formation grew into a substantial covering composed of yearly layers. As there were perishable materials down below, they subsided, so that the lime covering, having in places no support, gave way to the force of the water, or to the weight of boulders

hurled upon it, and water found admission between the lime sheet and the buried materials; as these were more moveable than the covering, they gradually washed out, and left the river to resume its ancient course beneath a covering of its own formation. Of course subterranean streams may excavate caverns; but if these streams owe their origin to percolation only, no large organic remains will be found in the caverns. If these are formed after the fashion of the Ghara caves, some organic remains may still be found, though waters have washed through them for years. When these lime formations withstand all the forces to which they are subjected, and have grown into large hills, the materials which formed the mould of the cave are still *in situ*, the perishable portions have changed into an oily, loamy soil; but bones, pebbles, and other materials are there mixed up with the stalagmite, which originally forming upon the surface of these materials, sunk and broke up as the supports failed, and remain, as Mr. Dawkins found them in Kent's Hole.

H. P. MALET

The Anglo-Saxon Conquest

I SAW, with perfect clearness, the grounds upon which Prof. Rolleston now rests his defence. It is assumed that we can identify, to a nicety, the precise period of an interment, having no recording date, that took place fourteen or fifteen hundred years ago. I need not contest or discuss this point, but I am at liberty to doubt it.

The Britons petitioned Rome for assistance in A.D. 446. Cerdic landed in 494; the *groans of the Britons* were a piteous wail of "old men and children." The interval, taking it at three generations, passed in constant warfare, would not restore the balance of youth removed by conscription prior to A.D. 418, which circumstance therefore remains in part as an element to affect the general average of assumed longevity in the time of Cerdic. I urge this, with all respect, not as affecting, in any way, the real facts discovered or expounded by Prof. Rolleston, with whose address I was very much gratified; but only as affecting certain conclusions sought to be founded thereon. A. HALL

Curious Effect of the Words "Carmine" and "Germinal Matter"

DURING the twelve or thirteen years that Mr. Huxley has performed the duties of Examiner at the University of London, it appears that he has been much disturbed by the frequent use the candidates have made of the words, "cell," "germinal matter," and "carmine." He says: "*I declare to you I believe it will take me two years at least of absolute rest from the business of an examiner to hear either of the words without a sort of inward shudder.*"* This is surely a very remarkable declaration on the part of the examiner, since it is extremely doubtful if hearing these words ever made anyone else shudder, and if any other words written or spoken ever produced an effect so exceptional upon Mr. Huxley.

LIONEL BEALE

Holly Berries

WILL some learned botanist, or Darwinian theorist, kindly inform me through the columns of NATURE why some holly berries appear obnoxious to birds? This is a great holly neighbourhood, and there are at present several trees actually loaded with ripe berries; the ground is also thickly strewn with berries beneath the trees, and yet not a single species of bird appears to eat them. Last winter the holly trees bore an abundance of berries, but the majority of the trees were stripped by the migratory *Turd*i, &c., as early as the beginning of February. I presume, in the "struggle for existence" these berries, obnoxious to birds, will stand a better chance of propagating and increasing that peculiar variety, and in course of time raise a distinct and well marked species.

Thurxton, May 23

HENRY REEKS

Origin of Languages

It seems to me that your correspondents, Mr. Taylor and S. J., have discovered merely imaginary differences between the origin of species and the origin of languages. Mr. Taylor sees an essential difference in the fact that in the one case the process "is carried on by the countless efforts of rational beings," whilst in the other case there is "*reasonless variation and selection.*"

* Address on Medical Education, *Lancet*, June 4th, 1870.